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Original Essay.

ART. I.—*Description of an Apparatus for the Treatment of Fractures of the Thigh and Leg. By N. R. Smith, M. D.*

(CONCLUDED FROM PAGE 317, NO. VIII.)

3. THE next circumstance necessary to the secure position of a fractured thigh, is the accurate support of both the leg and foot in such a manner that they shall not operate as levers to throw the lower fragment out of its proper relation to the other.—The weight of the leg operates with great power on the inferior fragment of the broken femur, and it is mathematically demonstrable that no force which can be applied to the sides of the thigh itself, can act with sufficient power, through the medium of the muscles, on the fractured extremities, to antagonise in any degree the tendency to displacement. The leg, therefore, must be carefully sustained, not merely by cushions and bolsters, for these are perpetually undergoing displacement, but by something which shall give uniform, unyielding, and yet easy support. That it may preserve the leg in a proper attitude in relation to the thigh, it must be securely fixed to the thigh-piece. The weight of the foot, when that organ is unsupported, tends to

rotate the leg and thigh outward, or inward,—the more so when the weight of the bed-clothes is added to it. This force is not great, but the constant operation of it, if not obviated, is certain at length to produce this result. Now the support of the leg and foot, by the apparatus here described, appears to me to be perfect. The firmness of the leg-splint, and the immobility of it on the thigh-piece, when the angle is fixed, render it impossible that it should drag upon the lower fragment of the femur. The foot is sustained with equal effect by the very simple turns which are made with the bandage round it, and round the extremity of the apparatus.

4. It is equally important that every apparatus for fractures of the thigh should be secured to some part of the body, that, if the body moves in any degree, and it is impossible that it should not, the whole apparatus may move in correspondence with it, otherwise, when the body moves, it will drag with it the upper fragment of the femur, leaving the other fragment and the leg, attached to the splint, in its former position. But if the splint is secured to the body, then, whatever movements of the latter may take place, the limb maintains the same relations, and the fracture is undisturbed. From this, too, it must be obvious that a light splint, neatly adapted and which readily accompanies the slight involuntary movements of the body, is far preferable to a cumbrous apparatus, which is so fixed to the bed, and so heavy, that it cannot move. Hence also the two-fold objection to the apparatus of Mr. Charles Bell, as it is commonly employed in this country.

The apparatus which I recommend is firmly secured by the hip-piece to the side of the body, so that when the latter changes its place, the splint moves in correspondence with it. Its lightness, also, and its convexity, where it rests upon the bed, are circumstances which conduce to the same result. But when I wish to make it even more perfect in this respect, I sling up the lower part of the splint, so that it may not touch the bed. Then, if the patient slides down in the bed a little, instead of crowding one fragment down upon the other, the

whole splint and limb will instantly move in correspondence, nor meet with the slightest resistance; and so in all other movements.

5. Another excellence belonging exclusively, I believe, to this apparatus, is the complete security of the limb, when reposing in the hollow of the splint, even though no bandage be applied, except to bind the hip-piece to the side of the body.—The bandage is only necessary for the purpose of sustaining the foot and giving firmness and strength to the apparatus. It also adds to the security of the member during sleep. But even when the bandage is applied, it will be observed that the limb is no where ligated by any strap, band, or bandage, the use of which is now pretty generally admitted to be injurious. The bandage being on the outside of the splint, the limb can no where be unequally constricted by it—the sides of the apparatus are merely compressed upon the limb. That the limb will swell more, if a bandage be not directly applied to it, is a lost fear—we may obviate it far more effectually by elevating the lower extremity of the splint.

There is scarcely any more prolific source of mischief in the treatment of fractures, than the direct application of bandages. Many years ago they were snugly applied at the place of fracture, for the purpose of moulding the callus, and shaping the new-formed bone. More recently, they have been used because our fathers have done so before us, and because they are supposed to obviate swelling. But I am persuaded that their effect is directly the opposite—it certainly is, whenever a bandage does not act with perfect equality, and I defy the most skilful dresser, to apply a bandage to a member varying in size from day to day, that shall not constrict it unequally, unless it be frequently re-applied; and this is impossible, without greatly disturbing the injured member. In some modes of dressing, in which bands are firmly applied to the neighborhood of the fracture, it becomes necessary to apply bandages below, to prevent the mischief which might result from its being bound but in one place.

All bands which press upon fractured limbs for the purpose of tying them to the supporting splints, do more or less injury. If possible, the weight alone of the member, sinking it into its bed, should maintain its proper form, and this is the case in the apparatus above delineated. There is no necessity here that the surgeon should perpetually watch for the relaxation of a band, or the displacement of a piece of the splint—nothing, which is necessary to the security of the limb, can give way.

Another great advantage consists in the facility with which the limb may be examined without the necessity of relaxing any part of the apparatus which gives support to the member. Indeed, as the bandage can be removed without inflicting the least pain, it will often be found salutary to remove it every day, and to use gentle friction upon the limb with the hand.

6. This apparatus is especially applicable to the treatment of compound fractures. It is so, first, because of the uniform and easy support which every part of the member receives; and, secondly, because it is so easily accessible. Even should the wound in the soft parts exist on the inferior part of the thigh, nothing is easier than to cut away a portion of the walls of the apparatus, opposite to the place of injury. This will enable us to dress it with perfect facility.

7. In cases in which it becomes necessary to employ permanently extending force, (and I believe that they are not numerous when this apparatus is used,) nothing can enable us, as it appears to me, to accomplish it with more ease than this. In the first place, there is always a gently extending effort made upon the thigh, from the attitude in which the member is placed.—The thigh is placed on one inclined piece, and the leg upon another; the leg, therefore, inclines to drag in one direction, and the thigh in the other, by their own weight. The leg will act on the lower fragment of the thigh to prevent its following the other, which sinks toward the bed.

But when a more powerful effort is required, the leg-piece must, as we have before described, be separated from that of the thigh, brought lower, and again fixed. This has the effect

of elongating the thigh-piece. The leg cannot slide upward, because of the manner in which it is sustained by the slings and bandages. We may also have the foot fixed to the extremity of the leg-piece.

The counter-extension is made with peculiar advantage, because it is effected by nearly the whole cushioned margin of the upper splint, applying itself very extensively to the perinæum, to the tuberosity of the ischium, andto the trochanter, diffusing its pressure extensively over the surface.

8. Another excellence which we claim, is the facility with which the angle of the apparatus is varied, the member being more or less flexed or extended. A little variation in this respect sometimes gives great relief, when a uniform attitude has become tiresome. This also renders the apparatus peculiarly useful when an injury has occurred in the vicinity of the knee joint, and when, after union is supposed to be taking place, we wish to prevent anchylosis of the joint by gentle flexion and extension.

Adversaria.

To Professor Smith.

Sir,—The following cases are designed for insertion in your Journal, if you consider them of sufficient importance to engage the attention of the public.

B. TICKNOR.

Art. 1.—*Amputation of the Lower Jaw, Translated from an Appendix to the Medicine Operatoire, of Sabatier.*

The subject of this case, on whom M. Dupuytren performed the operation of removing the lower jaw for the first time, was a coach driver, of the name of Lesier, and about forty years of age. In the year 1797, he began to experience a dull pain in the lower jaw, and a fungous excresence grew rapidly from the socket of the left canine tooth which had broken and removed. This tumor, to which the actual cautery was repeatedly applied, was always re-produced, larger and more painful than before. It degenerated at last into carcinoma; and when the patient, after much hesitation, came in 1812, to entrust himself to the care of M. Dupuytren, and determined to submit to an operation, he is represented to have been in a wretched condition, as the following description will show.

The cancerous tumor extended itself from the second molar tooth on the right side, to the angle of the jaw on the other; the root of the tongue was pushed backward; the teeth lacerated the tumor, from which distilled an acrid fœtid sanies. The lower jaw was increased to three times its natural size, and the tumor was deeply buried in its substance, which in fact was disorganized. The tumor, which was of a reddish colour, inter-

spersed with white spots, filled and projected beyond the mouth, which it kept open to as great an extent as the articulation of the jaw would permit. It appeared to be divided into three lobes, one of which projected between the dental arches; the other two elevated, one of them the right, and the other the left cheek. Aliment could only be introduced at the right angle of the mouth by the aid of the finger. Respiration was difficult, ptyalism profuse, mastication almost impossible, and speech scarcely intelligible. Nevertheless the appetite was not at all impaired, the cervical ganglious were not diseased, and the soundness of the patient's constitution removed all apprehension on account of the slow fever, from which he was hardly ever entirely free.

On the 30th of November, 1812, M. Dupuytren performed the operation, assisted by M. M. Breschet, Lebreton, and Lisfranc. In order to invigorate his courage, the patient had imprudently drank three pints of wine, a short time before the operation. He was placed in the same position as in the operation for cataract; and an assistant pressed the labial arteries against the bone. The surgeon being seated before the patient, seized the right side of the under lip with his left hand, while an assistant held the right side of the lip and extended it. An incision was then commenced at the middle of the lip, and extended down nearly to the os hyoides. By this incision two flaps were formed which were dissected, and detached from the tumor on each side, which remained untouched. The labial arteries, being imbedded in the muscles, were easily avoided, and the flaps being turned back and held by assistants, allowed the bone to be separated from the remaining soft parts, and its periosteum to be divided. The jaw being then firmly held, was divided on each side an inch from the angle by a smooth saw.

Until this period of the operation only a few drops of blood were lost; but in dividing the soft parts connected with the inner surface of the bone, considerable hemorrhage took place. It was not so profuse, however, as had been expected. The fingers of the assistants applied to the branches of the sub-men-

tal and lingual arteries stopped the effusion of blood; and the tumor was removed entire. The patient now became faint.—A ligature was applied to the sub-mental artery, and actual cauteries of a white heat were applied to the bottom of the wound and put a stop to the oozing of blood.

The powers of the circulation being restored, and there being no oozing from the wound, lint was applied to each stump of the bone, and the flaps were brought together. To facilitate the discharge of purulent matter, an opening was preserved at the lower part of the wound near the os hyoides, by inserting a little lint between its edges. The rest of the wound was dressed with lint, over which compresses were laid, and the whole secured by a proper bandage. At this time a fresh hemorrhage took place, and it became necessary to remove the dressings, and make another cauterization, more difficult than the first; after which the wound was dressed, and the patient walked to his bed.

The parts removed by this operation weighed a pound and a half. The jaw-bone was affected with exostosis, caries, necrosis, and was softened in many places. The fungus which grew from it, and which was deeply implanted in its substance, was hard, fibrous, and grated under the knife. It presented many ulcerations, below which its soft tissue had become lardaceous.

The symptoms which followed so serious an operation, were moderate. Those parts of the dressings which were detached by the suppuration, being removed on the fifth day, it was found that the portion of the divided integuments which had been brought into contact, were perfectly united. The pain and swelling were considerable; and there was a copious discharge of a mixture of pus and saliva from the inferior angle of the wound. The wound acquired every day a more healthy appearance. On the fifteenth day the escars were detached; the patient's appetite was perfectly restored, and there existed no disturbance in the system. Healthy granulations appeared in that part of the wound which had not united by the first intention; the suppuration diminished; the opening kept at

the lower part of the incision closed; and the wound advanced rapidly towards a perfect cure.

On the twenty-seventh day, the patient drove a carriage; on the thirtieth, two small fragments of bone were detached from the stumps, and fifteen days afterwards the cure was complete.

It was not without astonishment, that this man was then observed attending to his ordinary occupations, and presenting only slight traces of the loss of substance which he had sustained. He experienced very little difficulty in speaking, swallowing, and respiration. The soft parts became considerably firm and hard; the two stumps of the jaw-bone approached each other, and a kind of skin has been formed, which has almost entirely removed the deformity. At this time, (1824) M. Lesier is in the enjoyment of excellent health.

Results of the Amputation of the Lower Jaw.

M. Dupuytren has already (1824) performed this operation, in two different ways, eight times. One patient was affected, in consequence of it, with severe inflammation at the root of the tongue, and with that infiltration of the edges of the glottis, which is known by the name of œdematous angina. The patient died. In two others, the disease for which the operation had been performed, which was of a cancerous nature, returned, after appearing for many years to be cured, and proved fatal. But in five or six of the last cases in which the operation has been performed, it has been crowned with the most complete success, and a perfect cure has been effected. In some of these cases the whole of the jaw anterior to the angles was removed. Such was the case of a man who presented himself before the Royal Academy of Surgery for inspection, both before and after the operation. And, what is remarkable in all these cases, the two ends of the jaw-bone approached each other, and have been united by a kind of callus, in a firm and unyielding manner.

"This operation, therefore constitutes one of the most precious conquests of modern surgery. It has recently been performed by M. Sallemond, of Montpellier, on a subject who was freed by it from a cancerous affection, which had destroyed the lower lip, chin, and middle part af the jaw-bone.

ART. II.—*Aneurism of the Axillary Artery, cured by the Method of Valsalva.*

After making some remarks on what Morgagni says on this subject, Sabatier proceeds as follows:

"But however that may be, I have experienced the good effects of the treatment under consideration, in an officer who had an enormous aneurism situated before the humeral extremity of the clavicle, in consequence of a wound with a sword in the axilla. The danger of his condition was known to the patient, and he resolved to submit to any method of treatment which promised to lessen his danger, or prolong his life. I proposed to him the method of Valsalva, and he submitted to it without hesitation. After being bled several times and confined to his bed, he was subjected to the most severe regimen. His drink consisted of lemonade with a small proportion of sugar, together with a kind of mineral water, and the syrup of comfrey. He made a daily use of the alum pills of Helvetius, and the tumor was covered with a bag half full of the fine bark used in tanning, which was frequently wet with red wine. It was perceived after some time, that the tumor diminished, and that the pulsations in it were less distinct. This appearance of success supported the patient's courage, and induced him to persevere in the use of the means which have been mentioned; and he had the happiness to see the tumor reduced to a small hard tubercle, in which no pulsation could be perceived. The powers of the system were restored by degrees, and I have had the satisfaction to see the patient perfectly cured."*

* This method succeeds better in cases of Traumatic Aneurism than in others. N. R. S.

ART. III.—*A Case of Necrosis of the Tibia, which required an Operation, to free the Sequestrum from its involucra. By Henry Abbett, M. D. of Baltimore.*

In the early part of the spring of 1828, I was called upon to visit a lad about twelve years of age, the son of a highly respectable farmer of Bucks County, Pa., labouring under an affection which the family supposed to be inflammation of the knee joint. I found the knee swollen to double its natural size, extremely painful to the touch, with all the marks of high inflammatory action, and attended by symptomatic fever. No other position of the limb presented the least appearance of disease; and the patient when questioned upon the subject, referred all his sufferings to the above named joint. Under these circumstances I hesitated not to coincide in opinion with the parents of the child, and accordingly based my treatment, upon the view I had taken of the nature of the disease. I commenced by the free use of blood-letting, placed my patient upon his back with the limb elevated to relieve the blood vessels of the part; purged him, and kept him upon low diet. This plan of treatment was pursued for some time, and apparently with a very happy effect, for the swelling was gradually subsiding, the symptomatic fever abating, and the feelings of the little sufferer were becoming every way more comfortable. I now flattered myself that a speedy termination of the disease was about to be realized, but these hopes were of short duration, for upon a subsequent visit, the anxious mother informed me, that the swelling had gone down entirely in the night, but that her son had experienced one of the severest chills she ever witnessed; and she supposed that he would be attacked with a regular series of chills and fever. From some experience which I had had in diseases of the bones, in the practice of the Hospital and Almshouse of the city of Philadelphia, I was induced to expect in this instance the existence of some deep seated mischief; and although there was a total subsidence of pain, as well as of the previous symptoms, and a careful examination in the course of

the tibia, disclosed nothing that could warrant a conlusion that the bone was diseased, I ventured to communicate my suspicions to the family. The chill did not return, and all the other symptoms being absent, I ceased my attendance, after giving directions to the parents to advise me of any unnatural appearance on the diseased limb. In the course of a week or ten days I was again called to visit the child, with the information that matter had formed on the leg. I visited my patient immediately, and upon examining the tibia, in its course downwards, about one inch and a half below the joint, I distinctly perceived the fluctuation of matter, together with a solution of continuity of the bone. This was precisely the state of affairs that had been anticipated. I immediately plunged a thumb lancet through the integuments, and opened a passage for the pus which flowed freely; and upon passing a probe into the wound the nature of the disease was readily distinguished, by its coming into contact with the sequestrum or dead bone, and imparting to the hand that peculiar sensation, with which surgeons have become so familiar as scarcely ever to be deceived.—Having seen a case of this disease, in the hands of a highly respectable surgeon in Philadelphia, recover by the spontaneous efforts of nature, with the slight assistance of removing, by means of a pair of forceps, the small spicula of dead bone, as they presented themselves at the external orifice, I determined to adopt this course of treatment, although slow in its operation. After giving some directions with respect to his diet and exercise, I waited patiently the operation of nature. Matter continued to issue daily from the opening, over which was placed a small piece of sponge for the purpose of absorbing the discharge. In the course of six weeks, another artificial opening was made through the integuments about three inches below the former, which also discharged a considerable quantity of pus. Between the two openings a septum or bony bridge was distinctly perceptible. From a minute and careful examination made at this period of the disease, it was very evident that the bone was extensively involved, from about an inch and

a half below the knee, to near the ankle joint. I now based my hopes upon the powers of the system to support a long continued drain, until the sequestrum could be completely separated and thrown off. I ordered a nourishing diet, and such exercise as could have no injurious effect on the disease; my patient's health continued for some months very good, some small spiculæ were extracted from the orifices, but the great mass of diseased bone remained firmly enclosed in a new case, or osseous involucrum which had formed around it. Thus matters stood until the latter part of the month of October, when my patient's health began to fail. About this period he was attacked with hectic fever, the chills occurred very frequently, followed by flushes of heat, which were of short duration and observed no kind of regularity. They were soon followed by night sweats, and his strength wasted daily. I now placed him on the use of tonics, aided by such a course of diet as I thought would prove auxillary in supporting his strength. Porter, wine, bark &c., were freely employed, but all to no purpose; he was sinking fast under his complicated afflictions, and his situation now called imperiously upon me to save him from his impending fate. In order to extend to him, all the advantages that would result from the removal of the original cause of his sufferings, I determined upon an operation. To this his parents readily assented, after being informed that it offered the only hope to preserve his life. I accordingly proceeded to the operation—had him laid on his back, with the limb extended, and commenced by making a longitudinal incision with a scalpel through the integuments, from the upper to the lower foramen, and laid bare the newly formed bone between these two points. The integuments were next dissected from the bone on either side, so as to expose the whole of that portion of its anterior surface which was situated between the foramina. By means of Hey's saw, the bony bridge was cut away; two longitudinal incisions were made from the upper to the lower foramen, at the outer edges of the openings, and the piece of bone thus detached, removed. The superior portion of the sequestrum was now

grasped by a pair of forceps, but its extraction was opposed by some small spiculæ of new bone which it was necessary to get rid of. They were removed with a small chisel, and by this means I succeeded in enlarging the opening sufficiently to permit the dislodgement of diseased bone, which was readily accomplished. The cavity was carefully washed with tepid water thrown in from a syringe. The sequestrum, which is in my possession, was brought away entire, and measures four and a half inches in length. The superior part, for two inches, presents nearly a complete section of the tibia, having undergone very little change on its external surfaces; the cancellous or reticulated portion is entirely destroyed. The inferior portion has suffered considerably from the ravages of disease, and is very irregular in its appearance. The edges of the external opening were brought together, and retained in their situation by the interrupted suture, an opening being left for the escape of any matter that might form. A slight discharge continued daily for nearly two weeks, at the expiration of which time the wound healed. The hectic fever, night sweats, &c., left him, and I had the satisfaction to see my little patient restored to his friends with a very useful limb. But one of the most singular circumstances in the history of this case is, that the new tibia is one inch and a half longer than that of the opposite leg. The fibula retains its original connexion at its upper articulation, but is evidently displaced below, thereby forming a natural luxation. As a consequence of this new arrangement, the external malleolus is situated higher than its natural location. The tibia alone articulates with the astralagus, the fibula being removed to such a distance, as to prevent its having any agency in the motions of the joint: and the only inconvenience experienced by the lad, arises from the increased length of the new bone, which renders him somewhat lame.

Analytical Reviews.

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ART. I.—*A Treatise on Neuralgic Diseases dependant upon Irritation of the Spinal Marrow and Ganglia of the Sympathetic Nerve. By T. Pridgin Teale, &c. p.* 117, 1829.

(CONTINUED FROM PAGE 345, No. VIII.)

Neuralgia of the Heart.—It not unfrequently occurs that the action of the heart betrays signs of irregularity which cannot be ascribed to structural disease. When excited by mental emotion, or stimulated by exercise, it palpitates with unusual frequency and force; when the causes of its disturbance have ceased to operate, the irregularity continues unabated, and after variable intervals returns without any sufficiently obvious reason. Females seem to be more obnoxious to these "nervous palpitations" than males; as the complaint advances, the paroxysms become more frequent, are of longer duration, are induced by more trifling causes, and are separated by less distinct intermissions; and ultimately to speak or move is quite sufficient to induce them. These palpitations are often accompanied by pains in the heart and lungs, not very unlike rheumatism, which are sometimes seated in the arch of the aorta, sometimes pursue the course of its large vessels, and when they attack the bronchial tubes produce strong asthmatic symptoms. This state of the heart and lungs the author believes to depend "generally, if not always," upon a morbid state of the cervical ganglia of the sympathetic; but as the spinal cord is rarely healthy when these ganglia are diseased, it is not unusual to find that these neuralgic symptoms of the heart and lungs are complicated with darting pains along the cutaneous nerves of the head and neck, fixed pains around the shoulders, pains, numbness and tremors of the arms, and, what is somewhat singular, the left side is more frequently, as well as more violently affected than the right.

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palpitations dependant upon organic disease of the heart, by the absence of other symptoms which denote a change of structure in that organ; in hypertrophy, the pulsations of the heart are more vehement and more uniform; in dilatation, they are felt over an unnatural extent of the chest; when there is obstruction to the circulation from contracted orifices, from loss of function in the valves, or from morbid alterations of the muscular structure, there are generally, in a greater or less degree, blueness, œdema, &c. These symptoms, in general, are sufficient to distinguish the two affections; I will, however, add to them the stethoscopic distinctions enumerated by Laennec: 1. The heart is found to be of natural size; the sound, though clear, is not strongly heard over a great extent.—2. The shock, although apparently strong at first, has, in reality, but little impulse, for it does not sensibly elevate the head of the observer. The last sign he regards as most important, when, in addition to it, we consider the frequency of the pulsations, which is always greater than natural." 45.

The two succeeding cases are good illustrations of this form of neuralgia. Mrs. H. aged 53, had been for many years subject to palpitations and dyspnœa, which were supposed to depend upon the presence of water in the chest, which varied much in intensity at different periods, and became so severe in September, 1828, that they occurred repeatedly in the day, and during night not unfrequently awoke her. The action of the heart was very violent, the dyspnœa occasionally threatened suffocation, a wheezing sound was heard in the upper part of the chest, and when the hand was applied to it a peculiar vibration could be perceived. Each paroxysm lasted about 15 minutes, and in the interval the lungs and heart resumed their natural action. She was also annoyed with "fluttering" sensations in the arms, twitchings in the muscles, stiffness of neck, and a hoarse cough, unattended with expectoration. The third, fourth, and fifth cervical vertebræ were very, and a few of the superior dorsal were somewhat tender, in consequence of which leeches were applied with immediate relief, and the next day a blister produced still greater ease. On the 15th the palpitations and twitchings became more severe, another blister was recommended, and by the 22nd she felt quite well. Her complaint again returned on the 20th of December, and was again removed by leeching and blistering; she continued well until May, when another slight attack called for the employment of the same remedies, and up to the 29th August, of the present year, she has not only remained free from every neuralgic symptom of consequence, but has gained flesh and enjoyed good health.

Sarah B. aged 17, had been five months affected with pain in

the region of the heart, and palpitations which occur in paroxysms that are very violent during day, and less severe at night. The pain over the heart occasionally extended to the region of the lungs, the arms and scalp were often attacked with darting pains, an oppressive tightness was felt across the sternum, and there was great tenderness of the five superior vertebræ of the neck. By employing the ordinary remedies—leeches and blisters—and using the saline mixture for slight febrile symptoms, the palpitations and pains gradually disappeared, and in little more than a fortnight she was perfectly restored to health.

Whether, and how far these symptomatic derangements of the heart predispose to organic disease are questions of great interest; but the facts, which we yet possess upon these points, are so few, that it is impossible to enter into them with any prospect of a satisfactory result. When a muscle is forcibly and frequently exercised, an increased supply of blood and nervous energy is sent to it, that the demands made upon it in consequence of increased action may be fully satisfied. This extra supply is followed by extra growth, this extra growth gives rise to augmented energy, and in process of time both the form and function of the excited organ experience considerable change. In the brawny arms of the blacksmith, and in the muscular legs of the pedestrian, these remarks are strongly substantiated; and we are not aware of any principle in the animal economy, which prohibits our extension of this train of argument from external to internal organs. The heart is a muscle quite as much as the biceps, or gastrocnemius; and although it may not be as sensibly alive to mental stimuli as they are, it is subject like them to all the laws of muscular agency and organic life. Why, therefore, may not over excitement largely and long applied modify the anatomy of this organ as well as that of any other muscle, and why may not hypertrophy, or flaccidity of its walls succeed to an increased, or diminished condition of its function, as a limb emaciates from disuse or enlarges from exercise? Laennec admits the possibility of such consequences, while he has never met with any examples, and the author confesses himself unable to speak with decision either in favour or against it; so that at present the subject lies quite open to investigation, and we hope that its great interest will procure for it the attention it requires and deserves. There is no doubt but that organic disease of the heart may exist in connexion with neuralgic symptoms dependent on either spinal, or ganglionic irritation, that they may mutually aggravate each other, and that treatment, exclusively limited to the removal of the nervous irritation, has been found materially to relieve the organic disease. Whether, therefore, it be true or otherwise,

that what was mere neuralgia of the heart may ultimately issue in structual alteration, it is in every instance of heart-affection prudent, if not necessary, to attend to the condition of the spinal column.

"The treatment of nervous palpitations and neuralgic affections of the heart and lungs, has in general proved very unsatisfactory.—The means employed as remedies have been various in the extreme. These complaints have been treated by anodynes, antispasmodics, and tonics; by bleeding, digitalis, and prussic acid; by electricity, galvanism, and magnetism; and by irritants and depletory measures applied to the *anterior* parts of the chest. These means have generally failed to give relief, and some of them have even aggravated the disease. Not unfrequently has it happened that the unfortunate subject of nervous palpitations, after having tried in succession almost innumerable remedies, and having repeatedly changed his medical attendant, is obliged to endure with patience his distressing nervous companions, and console himself with the assurance that his complaint is '*seldom attended with danger.*' I feel considerable confidence in stating, that when the disease is treated upon the principle which I have laid down, namely, of referring the palpitations and pains in the heart to disease of the cervical ganglia, the most beneficial results will, in the generality of cases, be obtained." 47.

Neuralgia of the Stomach.—We believe it is now very generally admitted that an irregular condition of the stomach, depending on chronic inflammation of its mucous coat, is a frequent source of dyspepsia and its lengthened train of disastrous symptoms. This state may be relieved or removed by proper diet, occasional leeching and gentle sedatives. Another form of dyspepsia results from direct debility of the digestive organs, as a consequence of previous disease, and may be beneficially treated with tonic medicines. But there is a third variety of gastric ailment, which neither leeching and abstinence on the one hand, nor tonics and nutriment on the other can effect, yet the majority of its accompanying phenomena are so similar to those of the preceding cases, that they may be easily confounded. Its principal symptoms are impaired digestion, giving rise to acidity and distension; pain in the stomach, which may be confined to a small compass, or be diffused over the whole epigastric region; flatulency, depending not on the decomposition of indigested aliment, but on the secretion of air by a disordered stomach, pyrosis, pulsation in the epigastre, a corded sensation around the waist, soreness along the edges of the ribs, pain in the intercostal and abdominal muscles, and other indications of disease in the spinal nerves. Indigestion and gastrodynia, the first two of the preceding symptoms, are

common to gastritis and gastric neuralgia, and cannot be depended on; but the author thinks that the others are very diagnostic, more especially when attended, as they usually are, with tenderness of the spine. Flatulence from secretion, he believes to originate seldom in gastritis, pyrosis never, and a sense of constriction around the waist, soreness of the ribs and muscular pains rarely accompany it, while they are seldom absent from ganglionic disease. Præ aliis vero, says Lobstein, symptomatibus eminent flatuum extricatio, seu *pneumatosis*, e nervorum actione perversa oriunda. It must be remembered, however, that these points are not yet sufficiently well understood to admit of positive opinion, and it is certain that if disease of the ganglia continue long, the mucous membrane of the stomach, exposed as it is to continued irritation, may fall into an inflamed state, and thus may appear such an intermixture of idiophatic and sympathetic disease as shall require our treatment to be divided between the epigastre and the spine. Had the following case fallen under the care of some of our London brethren, the class of medicines which would have been adopted, it is not difficult to conjecture.

A married lady, aged 23, (June 5th, 1828) has been complaining for the last five weeks of a fixed pain in the left side of the abdomen, which is increased by pressure, an oppressive weight of the stomach after eating, constant weariness, extreme debility, a sense of constriction around the waist, which during night is distressingly severe, and an afflicting pulsation in the epigastre, which never ceases for a moment. All these symptoms are very much aggravated by taking food, and the stomach is so irritable after eating that it regurgitates by mouthfuls what has been swallowed, until it again becomes empty.—Slight flatulence and acidity are constant attendants on the digestive process. On examining the spine the 7th, 8th, 9th, and 10th dorsal vertebræ betray much tenderness, and some of those both below and above are rather uneasy when pressed.—Aching pains are also complained of in the legs, the skin covering the thighs is sore when rubbed, and prickling sensations are felt in the course of the saphena vein; blister to tender vertebræ; 10th, so much better as to say that she is "not like the same being." Pain and oppression of epigastre gone, pulsation diminished, food not rejected, is free from flatulence and acidity, lower extremities unafflicted with either prickling or pain; but as the spine was still somewhat tender a second blister was applied with the effect of banishing every morbid feeling.

An emaciated old woman (June 11th, 1828) has complained for several months of periodical pains across the epigastre, re-

sembling cramp, a corded sensation round the waist, sudden and copious discharges of air from the stomach, sometimes continuing for an hour at once, and pyrosis in a severe degree.—The 4th, 5th, 7th, and lower dorsal vertebræ were very tender under pressure, but there was no affection of the extremities.—A blister had been applied to the epigastre without relief; blister to the lower dorsal vertebræ. (19th) Pyrosis and sense of stricture gone, flatulence much diminished, fifth dorsal vertebræ still tender; six leeches to this vertebræ. On the 24th flatulence was trifling, and on the 2nd of July every symptom of disease was gone.

Angina Pectoris.—There are few diseases of which we know so little with any certainty as that to which Heberden gave the name of *Angina Pectoris*. The symptoms, by which it is described by various authors, are more remarkable for their variety than any thing else; the pathological products discovered after death are not much more uniform than the symptoms, and its treatment has experienced as many vicissitudes as the theories, which have been fabricated to explain its origin. Dr. Heberden observes that a disagreeable sensation in the breast comes on while walking, more especially after meals, and vanishes the moment that we cease to move. Dr. Walls describes it as a pain under the sternum, extending on each side across the breast, and affecting one or both arms where the pectoral muscle is inserted into the os humeri. Dr. Fothergill relates a case in which the leading symptoms were a sense of tightness around the chest, in a line with the mammæ, and a pungent pain under the left breast, which extended to the elbow of the left arm. Dr. Butter considers that the paroxysms are marked by dyspnœa and flatulence, and that relief is often obtained by eructation. Dyspnœa is excluded from the catalogue of symptoms by Dr. Parry, and he relates a well-marked case in which there was neither pain in the arms, or chest.—Dr. Blackall considers that palpitations are frequently characteristic of this disease, while Burns maintains that palpitations are incompatible with its existence. Parry describes it as depending on an ossified state of the coronary arteries, Desportes regards it as an affection which has its seat in the pneumogastric nerve, and Laennec believes it may originate in a diseased state of any of the nerves which supply the chest and neighbouring parts. The author agrees with these last writers in maintaining that angina pectoris has its seat in the nervous system; but, in place of considering with them that the nervous filaments are the parts diseased, "dans les filets que le cœur recoit du grand sympathétique," he only regards them as the

channel of intercourse, through which some disease in the spinal cord of sympathetic ganglia draws within its influence the functions of the heart. It is certain that symptoms of angina frequently occur and are removed, proving that there can be no serious disease in the structure of this organ; and how often have the coronary arteries been found ossified, where no syncopic symptoms had been visible? Looking upon the nervous filaments themselves as the morbid agents, treatment has commenced too generally at the wrong end, and blisters, issues and such remedies have been crowded on the chest; but the author thinks it much more consistent with facts to refer all the symptoms of disease to the nervous masses from which these nerves arise, and to direct our remedial measures to the spine in preference to the chest. Tightness round the waist, oppression at the epigastre, and pains in the intercostal and abdominal muscles he traces to the lower portion of the dorsal spine; flatulence and pyrosis to the lower thoracic ganglia of the sympathetic pairs; numbness in the neck and upper extremities to the cervical division of the spinal cord; palpitations and painful affections of the heart and lungs to the cervical ganglia.

"I have been induced to refer the various groups of symptoms which have been described as angina pectoris, to an affection of some portion or portions of the spinal marrow, and of the corresponding ganglia of the sympathetic, by the following considerations.

"1. The fact, as I have before observed, that most of the morbid phenomena exhibited in the extreme filaments of nerves, are seldom owing to disease in the nerves themselves, but to an affection of the nervous mass from which they are derived.

"2. The co-existence of pain on pressing some portion of the spine with the symptoms constituting angina pectoris; and the correspondence of the painful part of the spine with the particular symptoms which are present; namely, tenderness in the lower dorsal portion of the spine in conjunction with the stomach affection, constriction, &c. and tenderness in the cervical spine, with pains in the arms, breast, and shoulders, and palpitations.

"3. The relief obtained by local antiphlogistic measures to the spine; for instance, to the lower dorsal portion when the stomach is affected, and there is constriction, and to the cervical portion when there is an affection of the arms and palpitations." 108.

Our limits allow us room for only one example and it is an important one, as showing how closely connected the ordinary symptoms of spinal irritation are with those ascribed to angina pectoris, and tending to the presumption that they not only all depend on one common cause, but that they may pass and repass into each other, as this cause is moderate or intense.

A lady, aged 56, applied to the author (on the 18th of August, 1828) in consequence of general muscular debility, palpitations, sense of epigastric tightness, and flatulency.—Most of the cervical and some of the dorsal vertebræ being tender, leeches were first ordered and afterwards a blister, which gave such relief that in a few days she acknowledged herself better than she had been for many months. But on the evening of the 25th she was suddenly seized with coldness, an inexpressible sense of suffocation, tightness and oppression of chest, pain darting from the left arm into the elbow and down from the neck to the left breast, with a frightful feeling of impending death. A discharge of air from the stomach gave some relief, and a sinapism to the spine, warmth to the extremities, and internal stimuli soon restored her to a state of comparative ease. As the spine was again tender, and the symptoms at first complained of were again visible, blisters were recommended; but the patient, being obliged to return to her family, was removed from Mr. Teale's care, and the final result is unknown.

The diseases, which have now been described, appertain more especially to the heart, lungs, and stomach; but the author believes that other organs are occasionally affected in a similar way and from the same cause. The small and large intestines, the kidneys, bladder and uterus are not unfrequently the seat of neuralgia depending on spinal disease, and remedial by means directed to the tender vertebræ. Hodie certissime evictum est, savs Lobstein, quod tot numerosæ sensationes quæ in epigastrio percipiuntur, neque ad musculos, neque ad oagana gastrica sint referenda; sed magis ad plexum nervosum gangliosum trunco cœliaco insidentem. Autenreith likewise observes (in the first volume of the Jübinger Blœtter für naturwissenschaft und Arzneykunde) that in the bodies of those who have died of typhus, he has occasionally seen the abdominal nerves altered in appearance.

"It is of great importance to bear in mind the circumstance that these nervous affections sometimes accompany other diseases.—When the vertebræ, or intervertebral cartilages are inflamed, the neighbouring nervous tissues often participate, and neuralgic symptoms are the result. These nervous affections often constitute the most distressing part of the complaint, and, by proper attention to them the sufferings of the patient may from time to time be alleviated during the lingering progress of the vertebral disease. In fevers, symptoms of a neuralgic character often make their appearance, and aggravate the sufferings of the patient. The following case lately occurred to me. A young lady, having proceeded in a favourable manner for two or three weeks under common fever,

became affected in the afternoon with paroxysms of oppression in respiration, attended with severe aching pain and constriction round the waist. These symptoms returned about the same hour for four or five days, gradually increasing in violence until they became truly alarming; tenderness was discovered in two or three of the dorsal vertebræ, and a few leeches applied to the painful part, prevented the recurrence of the attacks. The fever afterwards pursued the usual course, and ultimately terminated favourably. Neuralgic affections of the scalp, connected with tenderness in the cervical vertebræ, often occur in fever, and are sometimes mistaken for pain of the encephalon. In phthisis, pains in the intercostal muscles, and oppression of respiration, are often of a neuralgic character, and readily admit of alleviation; the more formidable disease of the lungs, however, seems to predispose to their recurrence.

"Dr. Brown has observed neuralgic pains in the neck and scalp accompanying severe inflammatory affections of the fauces, and has also met with similar symptoms in conjunction with hepatitis. My own observation enables me to confirm these remarks of Dr. Brown. The principal neuralgic symptoms which I have observed in conjunction with hepatitis, are constriction across the epigastrium and pain or tenderness along the cartilages of the ribs. This pain is sometimes supposed to be seated in the liver when the right side is affected, but a precisely similar affection is as frequently met with on the left. I have known this neuralgic affection to be treated as hepatitis when there has not been any real evidence of disease of the liver. A patient is now under my care, who is suffering from hepatitis, as denoted by yellowness of the skin, bilious urine, clay-coloured fæces, and deep-seated tenderness beneath the cartilages of the ribs; during the course of this complaint, he was for several mornings in succession attacked, about five o'clock, with pain and constriction across the epigastrium which he compared to cramp, flatulent distention of the stomach and intestines, pain along the cartilages of the lower ribs on each side, and on pressing these parts a degree of soreness was felt; the attacks continued from one to two hours, during which great restlessness was produced. Tenderness was detected in the vertebræ, and a blister has quite removed the paroxysms.

"These circumstances point out the important fact that irritation of the capillary expressions of nerves may sometimes excite actual disease in the parts where the nerves originate. 55.

With a few remarks upon the probable connexion between colica pictonum and nervous disease, and a cautionary admonition to the reader against supposing that he has been either advocating an impregnable theory, or a practice which must ever prove infallible, Mr. Teale concludes his little work, and it were quite superfluous in us to repeat in plainer language than has been already used, the estimate which we have formed of its

execution. Suffice it to remark that, although we do not look upon his plan of cure in neuralgic disease as proof against disappointment, we consider it one of the most promising which has ever been recommended; and, although we must as yet hold his views of this disease as theoretical, until dissection supply the *experimentum crucis*, we are troubled with as few misgivings on the subject of its orthodoxy, as any can be during the absence of the *chief witness*. Mr. Teale has done much in a little book to elucidate an obscure order of diseases, and we have no doubt but that his labours will receive that recompense which talent and industry deserve.

ART. II.—*Thoughts on Febrile Miasms. Intended as an answer to the Boylston Medical Prize Question for* 1830. "*Whether Fever is produced by the Decomposition of Animal, or Vegetable substances; and if by both, their comparative influence?*" *By Charles Caldwell, M. D., Professor of the Institutes of Medicine, &c., in the Transylvania University, Lexington, Kentucky.*

THE Essay entitled as above is understood to have obtained the Boylston prize recently offered for the best treatise on this important subject. We deem it our duty, therefore, to give a concise analysis of its contents.

After some preliminary remarks on the importance and the difficulty of the subject, the author states that febrile miasms are of two kinds, contagious and infectious. The *contagious* are the product of living matter, the result of morbid secretion; a contagious miasm is secreted by the salivary glands of a rabid dog. The infectious are produced by changes in dead matter *presumed* to be of a chemical character, but not well understood. They are generated in no laboratory but that of nature.

But *contagious* miasms are also sometimes derived from dead matter, and these propagate their kind by the peculiar action which they induce—the small pox, for instance. This, when epidemic, is an atmospherical disease, and is thus imparted to individuals who have not been exposed to contagion. The poison is then not the product of secretion—whence it comes we know not. Small pox is more malignant when it is epidemic, than when communicated by contagion, and then often attacks the same individual a second time. The epidemic constitution predisposes the system, and the virus acts with the more effect on it thus weakened. Our author doubts whether, as is gene-

rally believed, the small pox was introduced into Europe by the crusaders. He says that when it first made its appearance in Europe it spread as an epidemic, and that its propagation did not depend on contagion—its progress being too rapid. It passed from city to city with great rapidity. Had it, (he says,) arisen from contagion, it must have been introduced much earlier, since commercial intercourse existed long before.

Our author here certainly asserts his belief with far more confidence than his arguments warrant. His reasoning is certainly weak in regard to its introduction from Asia into Europe. He says that, if it arose from contagion, it ought to have been sooner introduced. With precisely equal propriety might it be asserted that it should have occurred earlier if of epidemic origin, for, as he says himself, "there is nothing in Asia to generate it more than in Europe," and we know that it existed in Asia long before it occurred in Europe. We can easily imagine, too, why the slow and unfrequent commercial intercourse previously existing should not convey the disease by contagion. Persons thus contracting the disease would perish on their way, for those toilsome journies resembled rather pilgrimages, than the rapid voyages of modern times. But in large armies the disease would be kept alive by being imparted from one to another, until it was transported for hundreds of leagues. The fact, too, that the disease spread more rapidly at its first introduction than it has since done, also makes directly against his position. If contagious, this would certainly be the case while men were yet unacquainted with its nature, and the means of its communication. If it were of epidemic origin, subsequent epidemics should occasionally spread as rapidly as the first.

The more rapid diffusion of the disease, since it has assumed the variolid form, is also hostile to his doctrine. This disease is more mild—but an epidemic disease which spreads rapidly is usually severe and malignant. If it be contagious, it is obvious that a more mild disease, which individuals may even bear about them in their ordinary avocations, would be far more rapidly and in some instances unaccountably communicated.

Finally, when an individual contracts small pox, it is gene-

rally easy to trace its origin to contagious communication; and when this cannot be done, it is infinitely more difficult to prove the negative. Contagion is sometimes conveyed in the most astonishing manner, and accident has occasionally revealed a mode of communication which would otherwise have never been suspected. That small pox had a spontaneous origin is obvious, and that the same concurrence of circumstances does in the lapse of ages re-produce it, is probable. But there is certainly no sufficient evidence that it is epidemic, or extensively produced by a peculiar constitution of the atmosphere acting on the system. He says it is now often epidemic in America. Why then did it not occur among the Aborigines?

Dr. C. thinks that it is almost certain that the primitive origin of all contagious poisons must be referred to changes in dead matter. There would seem to be, (he says,) no other source, for they must have had an existence before they could have re-produced their kind in man, or any animal. Query, might they not have been generated in the human system by morbid secretion, as the poison of a rabid animal is?

The author inclines to the belief that measels are not contagious.

We must confess that we are astonished at the absurdity of his scepticism in this particular. The science of medicine will be scoffed at by old women, so long as physicians, in their efforts to generalize, attempt to reason away facts of every day occurrence. We positively know measels to be contagious, from the following facts. But a few weeks since, the writer attended a child which had been long affected with purulent ophthalmia. At length the disease began to yield, and we flattered ourselves that the cure would soon be accomplished.—But suddenly the child was affected with aggravated febrile symptoms—the appetite failed—the eyes became again extremely irritable—there was occasional sneezing, cough, and discharge of mucous from the nose—every symptom, indeed, which ushers in measels. But, as the child had for a long time been confined to an upper room which was visited by but few

persons, and by no other children, and as measels were not then prevailing in the immediate vicinity, we did not suspect the true nature of the affection till the eruption made its appearance.

For a time, no one of the family would admit that there had been the least exposure, and the case thus far was an admirable one for the non-contagionists. But after some days, it was discovered that a young woman, living in the country, had visited the child precisely two weeks before—that she was unwell at the time, and that on the next day, after returning to the country, she had the eruption of measels. The family distinctly remembered that she was unwell with symptoms which usher in measels, on the day that she visited the child.

In our own family it has been recently proved with equal precision. One of the children of the writer was accidently exposed to measels in May last. In two weeks after the exposure, (which was known,) the disease occurred. In precisely two weeks more, three other children of the family sickened with the disease.

Indeed, every nurse and mother has facts enough in her possession, relative to this topic, to overthrow all the dogmas of the non-contagionists from Hippocrates down to the present time.

We are not a little surprised that a doctrine so inconsistent with the plainest common sense and common experience, should be found in an essay to which a prize has been awarded by the very intelligent members of our profession in the city of Boston. Such an assertion, thus sanctioned, is calculated greatly to diminish public confidence in the certainty of medicine. Even admitting that the essay is meritorious in other respects, such an error should be regarded as covering a multitude of excellencies. It is a position which will generally be received with the same sort of indignant surprise, that a man of common sense feels when he hears the absurd philosophical dogma that there is "no heat in fire." Indeed, since the exploits of the fire-king, we think the latter assertion has certainly the advantage over that of our author.

To this class of *infectious* miasms are referred influenza, scarlatina, pertussis, typhous fever, pneumonia typhoides, plague, yellow fever, the sweating disease, dengue, &c. All epidemic and endemic diseases are from the same source. Epidemics depend upon peculiar constitutions of the atmosphere. One peculiar constitution gives rise to influenza, another to scarlatina, &c. &c. He states that two of these constitutions can never co-exist in the same place; for two portions of matter might as well occupy the same point of space.

This mode of analogical reasoning is very loose and equivocal. They who are of a different opinion, might with equal propriety aver, that two epidemic constitutions, *can* co-exist in the same place, as well as two colours in a coat, and, indeed, with far more propriety, for, as in the latter some threads may be white and others black, so may one part of a community be susceptible to one epidemic constitution, and another part to a second. Here again our author labours to subject facts to theory. Again and again we have seen two epidemics prevailing at the same time and place—measels and hooping cough, for instance,—dysentery and typhous fever. It has been often asserted with great confidence, and generally conceded, that two epidemic diseases cannot co-exist in the same individual; but even this doctrine has been overthrown by facts. Dr. Sheldon, of Litchfield, Con., than whom there is no better authority, asserts in a letter to the late Professor Smith of Yale College, that "in the course of the year 1807, the mumps, hooping-cough, and measels were all prevalent in this town, at the same time. The children of the Rev. Dan. Huntington were subject to these complaints. One of them had the *mumps, hooping-cough and measels* at the same time; another, measels, *and one of the other complaints. In each child the peculiar symptoms and appearances of each disease were exhibited and strongly marked;* and so far as I observed, progressed together, without any mitigation or suspension of either." If, then, not only two, but even three epidemic diseases may co-exist in the same individual, *a fortiori*, (admitting, as Dr. C. asserts, these diseases to be epidemic) two or more epidemic constitutions of the atmosphere may unquestionably exist at the same time, and in the same place. If the facts

stated by Dr. Sheldon do not give a quietus to the theory of Dr. Caldwell, we know not the force of truth.

Our author states with truth that the diseases which are so often ascribed to vicissitudes of temperature, are by no means thus satisfactorily accounted for. For if so, why do not similar vicissitudes produce similar diseases? That they do not is notorious; for at one time a sudden change from warm to cold produces epidemic catarrh, at another, epidemic peripneumony, and at a third, epidemic dysentery, or cholera. Nor does the intensity of disease correspond with the degree or suddenness of the change. The nature and intensity of the disease rather depend upon a mysterious quality of the atmosphere, which we term its peculiar constitution. The Doctor's opinions in this respect are neither new, nor so heterodox as he seems to fear they will be regarded. We think that Sydenham is sufficiently explicit on this subject. Dr. C. indeed, carries his belief in epidemic constitutions very far, and perhaps too far to be consistent with some of his other statements. If almost all diseases arise from epidemic constitutions, and but one of these can exist at the same time and in the same region, how happens it that we so often find ourselves, in ordinary practice, attending at the same time cases of dysentery, pneumonia, influenza, &c. &c.

Dr. C. declares his confident belief that scarlatina and hooping-cough are *not* contagious. In regard to the former there still exists a question, but in relation to the latter, his opinion certainly will be, as he seems to fear, "regarded as unfounded." That hooping-cough is perhaps sometimes generated without contagious communication, is probably true, and in this respect the disease is analogous to small pox; but that it is generally propagated by contagion is almost so palpable as to be self-evident. The Doctor remarks that the disease does not spread from a single family, or individual, in a regular and gradual manner. Many families in different places are simultaneously attacked, and its spread is irregular. It often passes over whole neighborhoods to attack others at a distance. Now, ad-

mitting it to be contagious, how easy is it to account for all this from what we may term the portability of this disease. A child having contracted it, not being sick enough to be confined, goes to a school frequented perhaps by fifty other children, and before the disease is suspected in him, communicates it to almost every family in a district..

He also observes that the fact, that neither measels nor hooping-cough can be propagated by inoculation, is a circumstance in favor of his belief—so, also, the fact that the contagionists cannot designate the tissue by which the poison is secreted.—But certainly this can be no more necessary to establish the contagious character of a disease, than to point out the precise origin of a miasm, is to prove the *epidemic* character of a disease. He says, too, that these diseases cannot co-exist in the same place. We are astonished beyond measure at the boldness of this assertion. We positively know that at this moment scarlatina, measels, variolid, and hooping-cough all exist promiscuously in the city of Baltimore. How absurdly then does he say that they (either of them) have exclusive possession of the atmosphere, by means of their miasms. We fully agree with the author when he states, "that farther arguments to the same effect might be adduced; but they are deemed unnecessary. If the foregoing are fallacious, and can be refuted, others no stronger would not be likely to have a better effect."

Let it be observed that our author denies the contagiousness of these diseases on speculative grounds, and from negative testimony, than which no logic can be more frail. We can bring, on the other hand, thousands of witnesses to declare that they have known the disease to be propagated by contagion. Which then should we believe? Certainly the latter, for theirs is direct and positive testimony. Let us illustrate this mode of reasoning. Let us suppose that five creditable witnesses were to declare to me that they had seen a person, well known to me, in the city of Baltimore, yesterday, and that a thousand others should say that they had not seen him. We should certainly, and without hesitation, believe that the individual had been in

the city, on that day; because the testimony of these five is positive, whereas that of the thousand is negative. He might have been in the city and they not see him. But it is impossible that the five should be deceived who saw and spoke with him.

Now we, who are contagionists, do not assert that measels and hooping-cough are always communicated by contagion, but only that they are generally so. When, therefore, the nullifiers adduce testimony to show that in certain cases (a thousand if you please) the disease was not imparted, it proves nothing, but all falls before a few facts like those which I have already stated. Let not the author lay this unction to his philosophy, that yellow fever was once as confidently believed to be contagious as are now measels and hooping-cough. There is no analogy whatever between yellow fever and these last. Yellow fever has never been known to diffuse itself beyond certain limits. It is a disease with which men of science have comparatively but recently become acquainted—it occurs at more remote intervals, and in comparatively but few districts. It is of so appalling a character also that the circumstances of its origin were never calmly contemplated. But measels and hooping-cough prevail every where, and at all seasons. They are diseases which are contemplated without apprehension, and children have been again and again purposely exposed to their contagion, that they might contract them at a favorable season, and they have then contracted them. The whole community are witnesses to the facts which prove their contagionsness.—This appears to us to be the true inductive reasoning on the subject.

Our authors remarks in relation to the supposed origin of miasms are judicious, and we give them entire.

"Of the origin of the several miasms which have been noticed, I have not spoken; nor need the ground of my silence on the subject be concealed. I can say nothing respecting it satisfactory to myself, or which I could expect to be deemed satisfactory by others. I am indeed ignorant of their origin. From what source, or by what means, the atmosphere becomes im-

mitting it to be contagious, how easy is it to account for all this from what we may term the portability of this disease. A child having contracted it, not being sick enough to be confined, goes to a school frequented perhaps by fifty other children, and before the disease is suspected in him, communicates it to almost every family in a district..

He also observes that the fact, that neither measels nor hooping-cough can be propagated by inoculation, is a circumstance in favor of his belief—so, also, the fact that the contagionists cannot designate the tissue by which the poison is secreted.—But certainly this can be no more necessary to establish the contagious character of a disease, than to point out the precise origin of a miasm, is to prove the *epidemic* character of a disease. He says, too, that these diseases cannot co-exist in the same place. We are astonished beyond measure at the boldness of this assertion. We positively know that at this moment scarlatina, measels, variolid, and hooping-cough all exist promiscuously in the city of Baltimore. How absurdly then does he say that they (either of them) have exclusive possession of the atmosphere, by means of their miasms. We fully agree with the author when he states, "that farther arguments to the same effect might be adduced; but they are deemed unnecessary. If the foregoing are fallacious, and can be refuted, others no stronger would not be likely to have a better effect."

Let it be observed that our author denies the contagiousness of these diseases on speculative grounds, and from negative testimony, than which no logic can be more frail. We can bring, on the other hand, thousands of witnesses to declare that they have known the disease to be propagated by contagion. Which then should we believe? Certainly the latter, for theirs is direct and positive testimony. Let us illustrate this mode of reasoning. Let us suppose that five creditable witnesses were to declare to me that they had seen a person, well known to me, in the city of Baltimore, yesterday, and that a thousand others should say that they had not seen him. We should certainly, and without hesitation, believe that the individual had been in

the city, on that day; because the testimony of these five is positive, whereas that of the thousand is negative. He might have been in the city and they not see him. But it is impossible that the five should be deceived who saw and spoke with him.

Now we, who are contagionists, do not assert that measels and hooping-cough are always communicated by contagion, but only that they are generally so. When, therefore, the nullifiers adduce testimony to show that in certain cases (a thousand if you please) the disease was not imparted, it proves nothing, but all falls before a few facts like those which I have already stated. Let not the author lay this unction to his philosophy, that yellow fever was once as confidently believed to be contagious as are now measels and hooping-cough. There is no analogy whatever between yellow fever and these last. Yellow fever has never been known to diffuse itself beyond certain limits. It is a disease with which men of science have comparatively but recently become acquainted—it occurs at more remote intervals, and in comparatively but few districts. It is of so appalling a character also that the circumstances of its origin were never calmly contemplated. But measels and hooping-cough prevail every where, and at all seasons. They are diseases which are contemplated without apprehension, and children have been again and again purposely exposed to their contagion, that they might contract them at a favorable season, and they have then contracted them. The whole community are witnesses to the facts which prove their contagionsness.—This appears to us to be the true inductive reasoning on the subject.

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pregnated, at different times, with the miasms of influenza, measels, scarlatina, hooping-cough, peripneumonia typhoides, and epidemic pleurisy, rheumatism and ophthalmia, physicians may conjecture, but cannot discover, in the present state of medical science. And as I have no wish to add to the number of existing hypotheses, I shall not hazard an opinion on the subject.

"But there remains a miasm more interesting and important to us than all the others, at whose existence and action I have hinted. Of this our knowledge is supposed, at least to be somewhat more extensive and accurate. I allude to the miasm of marshes, to whose influence is attributed every modification of autumnal fever. Nor is its action limited to the complaints of autumn. Throughout the year it is busy, in some climates; and even in our own, we feel its effects in spring and summer.—Nor are we free in winter, from its lingering action on those who had suffered from its influence during the preceding autumn. Thus does it run perpetually its circle of mischief. I proceed now to the consideration of this poison, and purpose to speak of it somewhat in detail.

"Of all febrile miasms, that of marshes is the most ancient, universal, and destructive. In these respects it has no rival.—If, once in a term of several years, the miasms of small pox, measels, influenza, and scarlatina, overrun tracts of country of considerable extent, that of marshes exhibits its effects in every populated country of the globe, during a portion at least of every year. All other miasms appear, from their history, to be comparatively of modern date. The ancients had no knowledge of them; or if they had, they have failed to leave a record of it. But the existence of marsh miasm is coëval with that of the human race. If the views entertained of its origin be correct, its commencement must have been contemporaneous with the first decay and dissolution of animals and vegetables. According to the present system of physical influences, its production seems to be as necessary, and as much in harmony with the laws of nature, as the descent of ponderous bodies, or the growth of plants. Since the first establishment, then, of the present order of things, it must have existed and produced its effects.

"In our most ancient writings, those effects, if not expressly recorded, are virtually alluded to. The plagues of Egypt were as much the offspring of the miasm of the Nile, in the days of Pharaoh and Busiris, as they are at present. Since the subsidence of the first inundation of that river, its banks must have been a hot-bed of this febrile poison. Wherever, in the

habitations of Israel, or among the nations around them the pestilence "walked in darkness," the same virus was present.

"The pestilence which desolated the Grecian camp, on the plain of Troy, is ascribed by the poet to the resentment of Apollo; but the philosopher derives it from the miasmatic exhalations of the Simois and the Scamander."

We conceive that Dr. C. carries his doctrine of miasms quite too far, when he ascribes to these epidemic constitutions, *all choleras*, and most of the diarrhœas and dysenteries that destroy such myriads of the human race,—also bilious cholic, jaundice, hepatitis, dropsy, enlargement of the spleen, and several other chronic complaints.

Our author's remarks on the history of the doctrine of miasms, we deem it expedient to pass over, merely observing that it is only within about 150 years that the subject has received proper attention, and that the first who distinguished himself by its investigation was Lancisi, of Rome, who was born in the year 1654. Nor do we deem it necessary to analyse his argument in favor of the existence of miasms, for we believe that our remarks will fall into the hands of but few who are so sceptical as to deny them; and yet the scepticism of such would not be at all more surprising than that of Dr. C. himself, in relation to contagion.

The author does not essay to explain the essential nature of the miasmatic poison. No eudiometrical test has yet been discovered sufficiently delicate to detect the lurking agent. A thousand chemical principles have, it is true, in turn been conjectured to be the basis of the secret poison.

The author next proceeds to reply more directly to the question proposed, "Whether Fever is produced by the decomposition of animal or vegetable substances; and if by both, their comparative influence?"

To reply to it fully, he does not attempt, deeming it impossible. He premises his reply by remarking that most of the lower order of animals, especially such as inhabit the water, and burrow in the ground, have nearly the same chemical constitution as vegetables. He thinks, therefore, that their products

from chemical changes must be very similar. It is but rarely that the larger animals, whose chemical composition is more peculiar, perish and are decomposed in sufficient numbers to influence the chemical constitution of the atmosphere. It is chiefly with reference, then, to the decomposition of the lower order of animals that the reply is to be made.

Typhous fever, however, the author regards as springing exclusively from animal matter exhaled from the human body, undergoing the changes which often occur in prisons, ships, and other impure and badly ventilated apartments. This differs from contagion, in this respect, that the matter when exhaled is perfectly harmless, but by spontaneous chemical changes becomes a virulent poison, when no longer subject to the powers of life; these, however, not being the result of mere putrefaction. We know nothing of the nature of the poison but by its effects.

In reply to the Boylston question, the author avers that marsh miasm, the remote cause of bilious fever, is the product chiefly of vegetable matter in the process of decomposition, but not of *real putrefaction*; for the poison exists where no signs of putrefaction appear. His language is here somewhat ambiguous.—What is the putrefaction of vegetable matter? certainly nothing but its *chemical decomposition*—the separation of those chemical principles which compose it, and perhaps their re-composition. When gases which offend the sense are produced, we are accustomed to term this chemical decomposition, putrefaction. What does the author mean, then, by asserting that marsh miasm is the product of vegetable decomposition, but *not of real putrefaction.* If he had said that, in the decomposition which takes place, the production of odorous gases is not necessary to the generation of miasms, we should understand him.

But here we feel ourselves constrained to throw down our glove and at once challenge, in all courtesy, the correctness of our author's main position, that even the mere chemical decomposition of vegetables can alone produce marsh miasms. There must exist some quality of soil or climate, or some peculiar

chemical constituent of the water, which aids the recomposition, or no miasmatic poison can be generated. And here let us produce another of those obstinate facts which mar the integrity of so many medical theories—a fact which all the sophistry on earth cannot put down or circumvent.

On the waters of the Connecticut river, a stream 300 miles in length, with many marshes, small lakes, and ponds communicating with it, such a thing as intermittent or remittent fever is never known; and yet vegetable putrefaction is abundantly taking place, since vegetation in the valley of the river is exceedingly luxuriant. Forty or fifty miles to the west of this river, however, *in the same latitude*, on the waters which fall into lake Champlain, both intermittent and remittent fevers are extremely common. These facts we know from personal observation, for we have resided for years both on the western and on the eastern side of the green mountains. The facts are known to thousands who are now residing there, and to them we appeal. We will here adduce also the testimony of an individual who practised for forty years in the vicinity of Connecticut river, probably more extensively than any other individual ever did.—The late Professor Smith of Yale College states, in his Essay on Typhus, that, "On the Connecticut river from North Hampton, in Massachusetts, to its source, a distance of more than two hundred miles from north to south, and on all its tributary streams, on both sides, for an hundred miles in width, there has been no instance of any person's having contracted the intermitting fever, from the first settlement of the country to the present time."

Our author states that bilious remittent, and intermittent fevers which prevail in the country, are perhaps universally the product of vegetable decomposition—sometimes, undoubtedly, aided by the decomposition of those lower orders of animals, whose chemical constitution so much resembles that of vegetables. The yellow fever of cities, however, he believes to be often the offspring of animal putrefaction. He adduces many instances in which this fatal pestilence, occurring in the city of

Philadelphia, was fairly referible, at one time to putrefying oysters—at another to damaged hides. To those who aver that, in many instances, great quantities of animal matter have been decomposed without the production of such disease, he makes the following remarks, which will be found to apply with singular felicity to his own reasoning against the contagiousness of certain diseases. "To sound etiologists it is scarcely necessary to observe that this form of reasoning is not only inconclusive, but exceedngly hazardous. *Its character is negative.*—It virtually contends that, because an event has not occurred under one set of circumstances, it cannot occur under another."

The author candidly admits the many difficulties which encumber his belief in relation to the origin of yellow fever.—His opponents ask "if such are the sources of yellow fever, why does it not prevail every summer?" To this he replies, that the warm weather of every summer is not sufficiently intense, or protracted, and a congenial epidemic constitution of the atmosphere does not always exist. He might also have stated that, in cities of southern latitudes, in which the causes of yellow fever are found, it does prevail every year, and, indeed, in some tropical cities, almost constantly. Against his belief, too, that the disease often arises from obvious nuisances, it is averred that, when the nuisance is removed, still the disease continues to be propagated in the same place, and perhaps more widely than before. To this he finds it difficult to reply, but at length comes to this conclusion, that from the damaged fish, corn, hides, or other putrefying mass, the miasm first goes forth, "and the disease then begins in its immediate neighborhood. As the season advances, the common impurities of the streets, which also consist of animal or vegetable substances, or of a mixture of both, are brought into a similar state of preparation. In them, therefore, the same process is excited and the same changes are produced." It is a little singular that after this statement, he condemns the belief of some pathologists that the miasm, first emitted from the original nuisances, may act as a ferment to excite, in other masses of filth, the

process necessary for its own production, and that by its operation on dead matter, it thus contributes to its own continuance. This he says is "only a conjecture, unsupported by fact."—But, in the name of good logic, where is the difference between this assertion and his own, just stated, that the disease begins in the neighborhood of the nuisance, but that soon the common filth of the street is "brought into a similar state of preparation." Brought into a similar state of preparation by what? It cannot be by the ordinary influence of heat &c., because this operates simultaneously on every section of a city; but the epidemic influence diffuses itself gradually from the centre where it first orignated, creeping by inches along the deserted streets. If his statement means any thing, it means that "they are brought into a similar state of preparation by an assimilating influence in the first cause."

Dr. C. next proceeds to inquire, *what are the agents requisite to the production of marsh miasm?* They are (he says,) in brief, heat, moisture, and dead organic matter, existing together in due quantity and proportions. In what manner these agents reciprocally act upon each other is not known. Marsh miasm can never be generated when one of these is absent—not, for instance, when heat is wanting, nor when moisture is absent.—Perfect aridity is inconsistent with it.

The author quotes, to condemn, the opinion of Dr. Ferguson, that a paucity of water is indispensable to the production of marsh poison, on all surfaces capable of absorption. To the opinions and facts presented by Dr. Ferguson (for which we refer to that gentleman's writings) he opposes those of Dr. Lind —indeed, he might adduce those of almost every pathologist, for Dr. Ferguson's opinions are as singular as some of his own doctrines. Moisture is undoubtedly necessary to the production of miasmata, although Dr. F. presents us with some troublesome facts in relation to this matter. Too much water, however, by diluting the products of decomposition, seem to hinder its production. Alluvial earth, which always contains a considerable quantity of vegetable matter, when exposed to the ac-

tion of heat and of the air has often been observed to be a prolific source of miasmatic exhalations. The author states that in Natchez, in 1820, the police of the city had occasion to excavate certain streets to the depth of six or eight feet, and with the alluvial earth, thus removed, hollows were filled up in other places. This was exposed to a burning sun and heated atmosphere, and gave rise to a miasm of a most malignant and desolating character.

Next the Dr. inquires—"How far does miasm, travel from the place where it is produced?" We are not in possession (he says) of a sufficient number of facts to render a complete reply to this question. He declares it to be his belief that miasmata are never conveyed farther than a mile from the place of their production. He believes it to be capable of being conveyed through the atmosphere by the wind, in proof of which he adduces facts too familiar to need repetition here.

We shall proceed no farther in the analysis of Dr. Caldwell's Essay, since the remainder contains doctrines less peculiar to himself, and facts which are familiar to most readers. We have taken the liberty to discuss the author's doctrines with perfect freedom, though we trust not disrespectfully. His Essay certainly contains much that is valuable, and even his errors are maintained with not a little ingenuity. Those errors, however, are in our humble opinion so inconsistent with sound pathology, and so important in regard to the safety of the community, that no conscientious reviewer can reserve his sentiments. They are so numerous, also, that we cannot but repeat our surprise that the Essay has won so much of the admiration of our eastern brethren, and taken the palm from among them. We infer from the Doctor's own language, that he will neither be surprised nor offended that his opinions have thus been assailed. If they are the golden truth, the coals which we have heaped upon them will only cause them to shine the brighter.

Abstract of Foreign Medicine.

PRACTICE OF MEDICINE.

Blake on Delirium Tremens.—An Essay on D. T. by Andrew Blake, M. D., is reviewed in the October number of the Edinburg Medical and Surgical Journal. The Doctor rejects the name which common consent seems to have conferred on the disease, and coins one of his own, *Erithismus Ebriositatis.* He defines it thus:—"Indirect general debility, succeeded by a morbid increase of action in the brain and nervous system, which is attended with delirium, and terminates generally, either in sleep and subsequent health, or in death from collapse, or effusion on the brain." This we believe to be correct. The symptoms of the disease being familiar, we shall pass over what is said of them, in the analysis from which we quote.

In a case which was the subject of post mortem examination, the author discovered the following appearances. The brain appeared healthy, except that a small quantity of lymph was thrown out between the dura mater and the arachnoid membrane. There was serous fluid in all the ventricles, but more especially the lateral. The choroid plexus was not turgid—viscera of thorax and abdomen natural—liver small but healthy.

In the treatment of the disease, Doctor B. urges careful attention to the distinct stages of the disease. In the first stage, when there prevails gastric derangement with nausea and vomiting, he uses effervescing draughts, with tinct. opii., ten drops, every second hour, with emmollient anodyne enemata. In the intermediate hour, he gives an ounce of rum, with warm water and sugar—warm bath, or cold effusion according to strength of patient, and probability of producing re-action—cold for young and vigorous, but warm for feeble and old. He also advises anodyne frictions on the epigastrium—that the head be shaved and rubbed with volatile liniment—blister on the neck.

When the stage of nervous re-action and excitement supervenes, he advises the administration of full doses of opium, supporting the system with brandy, wine, ether and antispasmodies. To these he adds Dover's powder 6 grains, and calomel 2, every two hours, till the system becomes affected, or the disease yields. The mercury he uses for the purpose of equalizing excitement, and removing obstructions. The warm bath he continues for the same purpose. Cold applications are made to the head.

Uterine source of Neuralgic Pains.—From No. 25 of Johnson's Journal, we analyse and condense a case illustrating a pathological circumstance just named. A female, æt. 25, irritable, liable to palpitations, and to frequent pains in head and side—her monthly periods irregular—discharge rather profuse, and sometimes clotted, had her catamenia suppressed by application of cold water to the legs. In two days, was seized with pain at the præcordium, but which soon fixed itself in the lower part of the abdomen. February 11, came to Guy's Hospital. Pain very severe—belly very tender—bowels unmoved—pulse 120—tongue slightly furred. V. s. 16 ounces, purge of extr. of colocynth and calomel—fomentations. The extr. was given with the hope of reproducing catamenia. Evening—great aggravation of pain from operation of pills—6 drachms castor oil.

Bowels freely moved—next day, better—severe pain occurring only at intervals, and chiefly in region of sigmoid flexure. Next day, had a slight re-

turn of catamenia, and morning after free from pain. Catamenia ceased again in the evening and pain returned This subsided under former treatment; but on 3d day after, she had acute pain under the margin of the ribs of the right side. This at first appeared like pleurisy, being increased by inspiration; but it varied much in intensity, and at intervals was much aggravated, which, with the history of the case, showed it to be neuralgic. It was relieved by a blister and the conium.

Production of Worms in the Human Body.—Professor Scouler of the Andersonian University, advances an ingenious and interesting hypothetis in relation to this subject. He first shows that two sexes are by no means always necessary to reproduction, for some animals may have an organ cut away, and it will be reproduced by the nutrient vessels. Others may be cut into many pieces, and each portion will grow into a perfect animal. He thinks, therefore, that we may advance a step further and maintain with plausibility that entozooa, or worms, may be produced by the animal which they inhabit. When we consider the variety of tissues which the nutrient vessels are capable of elaborating in the living system, it seems not improbable that the same vessels may form other structures of far greater simplicity. Tumors and tubercles he supposes to be organized by a preternatural exercise of the generative powers of the vessels, which form the healthy organs. These he says are something analogous to worms, and have, like them, a kind of life distinct from that of the rest of the system. The analogy of hydatids is still more striking, for they are admitted to be parasitical animals. Mr. Carmichael maintains, with great ingenuity, that cancer is a parasitical animal, preying upon the living system. Now, Mr. Scouler avers, that worms are produced, not by the fluids of the cavities, but by the generative powers of the vessels themselves.

The principal objections to this theory are, that many worms are organized as male and female, and have distinct organs of generation. It may be presumed, too, that if produced in the fortuitous manner described by Professor S. they would have nothing regular or determined in their forms.—We condense the above from an article in the last number of the Medico-Chirurgical Review.

We would direct the attention of our readers to our strictures on the doctrines advanced in the Prize Essay of Dr. Caldwell. If it contains that which can be shown to be erroneous, it should certainly be promptly exposed, since the Boylston Censors have conferred upon the paper a character, and degree of influence, which, of itself, it might never have achieved.

With the present number, our subscribers will each receive a lithographic portrait of the late lamented Professor Wells.—For it, we are indebted to our distinguished friend, Dr. George C. Shattuck of Boston, who, as a token of respect and affection for the memory of him on whose like we shall rarely look again, has kindly presented the editor with a sufficient number of impressions to supply the journal.